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Reply to Carl J. Rossi's Comment on "Proton beam and prostate cancer: An evolving debate" by Anthony Zietman [Rep. Pract. Oncol. Radiother. 2013;18:338–42]



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This letter¹ makes several thoughtful comments about the complex history of proton beam development in the USA. It is indeed true that money is not the only driver and technical limitations, particularly field size, were also a factor. Having said that, there are plenty of other smaller tumors on which efforts in PBT could have been concentrated. The conjunction of the "PSA Revolution" with its rising tide of new prostate cases and the rise of PBT inevitably brought the two together.

IMRT was never "held to account" for its rapid expansion and the cost consequences have been enormous. While I do believe that an RCT should have been performed for at least one disease site as proof of principle it simply hasn't happened. The history of US health policy does not make one optimistic that a trial will happen now. CMS and other major US insurers are much more wary of making the same mistake again and, fairly or unfairly, PBT will be held to a higher standard.

The RBE is certainly around 1.1 and the fact that there was no excess of grade 3 toxicity in PROG 9509 would seem to support that except that 60% of the dose in this trial was given with photons. A subsequent trial giving 82 Gy use proton beam monotherapy hit, in our opinion, upon a dose limit.² Photons have been used to deliver even higher doses successfully and this does reopen the RBE question. At ultra-high doses small uncertainties in RBE may matter profoundly.

Thankfully we do have the new randomized trial comparing IMRT and PBT as monotherapies which is accruing well. The trial does allow, and is stratified for, scanned beam technology in the proton arm and so we will indeed be comparing the best with the best.

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Conflict of interest statement

None declared.

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